

## PARA HYDROGEN EQUILIBRATION IN THE ATMOSPHERES OF THE OUTER PLANETS

Barney J. Conrath  
NASA/Goddard Space Flight Center

*The thermodynamic behavior of the atmospheres of the Jovian planets is strongly dependent on the extent to which local thermal equilibration of the ortho and para states of molecular hydrogen is achieved. Voyager IRIS data from Jupiter imply substantial departures of the para hydrogen fraction from equilibrium in the upper troposphere at low latitudes, but with values approaching equilibrium at higher latitudes. Data from Saturn are less sensitive to the ortho-para ratio, but suggest para hydrogen fractions near the equilibrium value. This behavior can be understood, providing the para hydrogen equilibration time exceeds the upper tropospheric radiative time constant. Such would be the case if the equilibration time were that for pure hydrogen ( $\sim 10^9$  s) on both planets. Above approximately the 200 K temperature level, para hydrogen conversion can enhance the efficiency of convection, resulting in a substantial increase in overturning times on all of the outer planets. Currently available data cannot definitively establish the ortho-para ratios in the atmospheres of Uranus and Neptune, but suggest values closer to local equilibrium than to the 3:1 "normal" ratio. Modeling of sub-millimeter wavelength measurements of these planets suggest thermal structures with "frozen equilibrium" lapse rates in their convective regions. Measurements from the future Voyager encounters, along with improved ground based observations, should lead to a better understanding of the importance of para hydrogen equilibration in the atmospheres of Uranus and Neptune. Laboratory determinations of para hydrogen equilibrium times in the presence of ammonia ice particles are needed to improve our understanding of the hydrogen thermodynamics on Jupiter and Saturn.*

Let me begin by outlining what I will attempt to cover this morning. Basically, I'll start off with a brief introduction to refresh your memory on the thermodynamics of molecular hydrogen and the story on ortho and para hydrogen and how it pertains to the atmospheres of the outer planets. I will cover a little bit of the background of some of the early work of various people, and let me apologize in advance if I forget to give people appropriate credit along the way. I will describe the observational results for Jupiter and Saturn, particularly some work that Peter Gierasch and I and others have attempted to do with the Voyager IRIS data. I'll look at the implications as we see them for some of the dynamics problems on hydrogen atmosphere planets. I'll say a few words about what we know about Uranus and Neptune. Finally, I will attempt to summarize the current outstanding questions in this area.

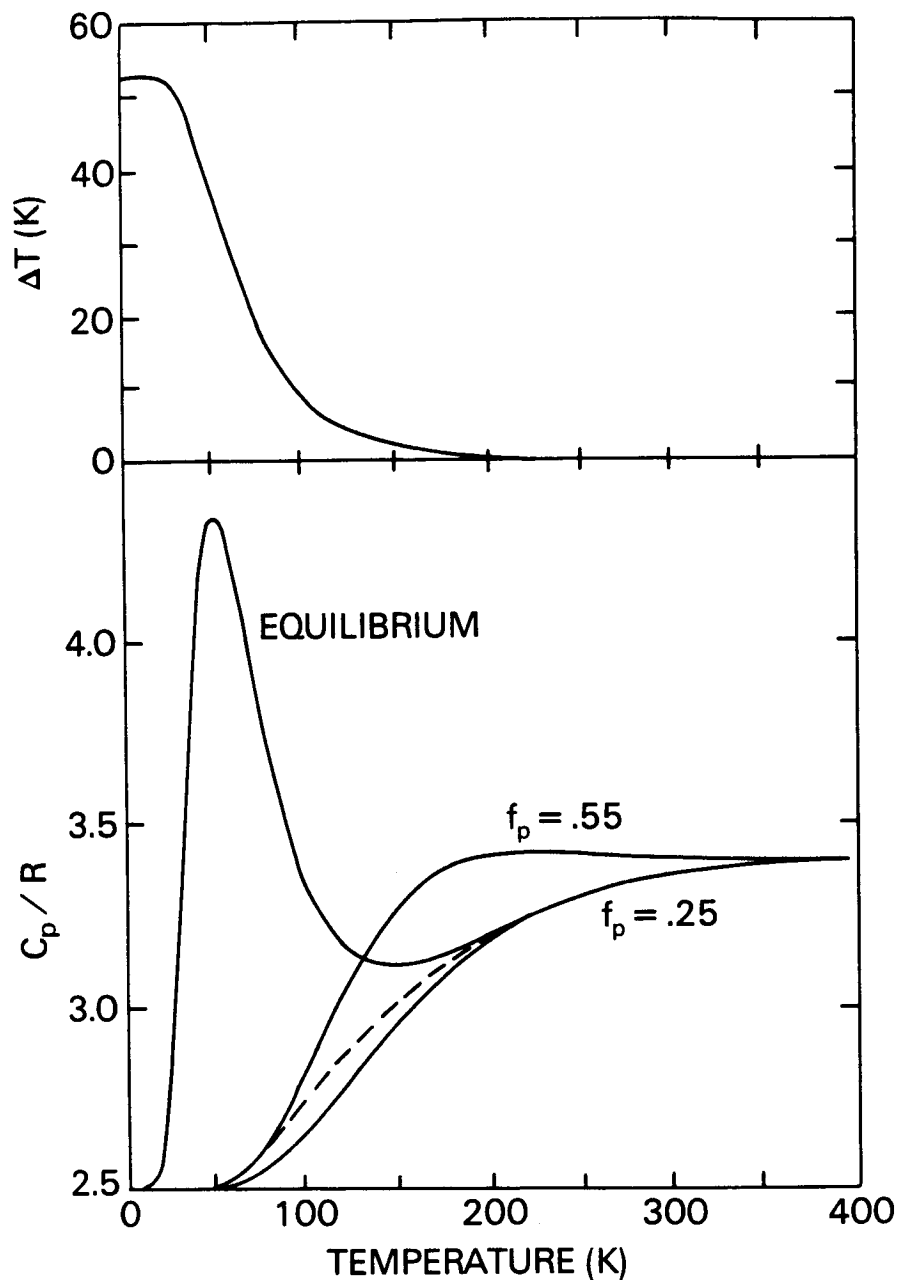


Figure 1. Temperature dependence of relevant thermodynamic properties for a mixture of 90%  $\text{H}_2$  and 10% He. The upper panel indicates the temperature increment  $\Delta T$  which results from the equilibration of a parcel of normal  $\text{H}_2$  at a particular temperature. The lower panel shows the nondimensional specific heats for fixed para hydrogen fraction  $f_p$  compared with that for equilibrium hydrogen. The dashed line represents the specific heat which corresponds to the situation where  $f_p$  is set at equilibrium value for the local temperature.

First, let me remind you of the story on molecular hydrogen; there are the ortho and para states. The ortho state corresponds to parallel proton spins, and the para state has anti-parallel spins. The ortho state is essentially a triplet spin state with a spin statistical weight of three; the para state has a spin statistical weight of one. The spin states are quantum mechanically coupled to the rotational states of the hydrogen molecule in such a way that the para state is associated with even rotational quantum numbers,  $J$ , and the ortho state has odd rotational quantum numbers. Now the  $\Delta J=1$  transitions are highly forbidden in pure hydrogen. Therefore, there tends to be very little communication between the ortho and para states. Thus, if one prepares a sample of hydrogen at a temperature of approximately 300 K or higher and brings it into thermal equilibrium, the ortho-para ratio is essentially the ratio of the spin statistical weights, or three to one. But as the sample is then cooled down to lower temperatures in the case of pure hydrogen, instead of achieving the ortho-para ratio of the local thermodynamic equilibrium, it tends to retain the 3:1 (so-called normal) ortho-para ratio for long periods of time. In the case of pure hydrogen, ultimately there will be an equilibration between the ortho and para states just due to the very weak paramagnetism of the hydrogen molecule itself, but on a time scale (under conditions, say, of the 500 mb level on Jupiter) estimated to be about  $10^9$  seconds. This time scale decreases as the pressure increases as we get to deeper levels, and the equilibration time should become shorter even for pure hydrogen.

Now, there are catalytic processes which can greatly reduce this equilibration time. For example, the presence of paramagnetic sites on the surfaces of solids will greatly reduce the equilibration time, and this has possibly important consequences in the atmospheres of the outer planets since the cloud particle surfaces may in fact provide some catalysis, which will make the equilibration go much faster. At any rate, this characteristic of hydrogen has some profound spectroscopic and thermodynamic implications.

Hydrogen can have a number of different specific heats depending on the conditions occurring and the resulting degree of equilibration of the ortho and para states. Figure 1 illustrates the specific heat as a function of temperature for a mixture of 90%  $H_2$  and 10% He. The curve labeled "equilibrium" corresponds to the limiting case of rapid conversion and hence strict equilibrium between the ortho and para states. In addition, you can have a specific heat associated with constant para hydrogen fraction,  $f_p$ . The para hydrogen fraction of 0.25 corresponds to the 3:1 ortho-para ratio of the normal or high-temperature limit. The dashed line represents a case in which one has a specific heat at constant para hydrogen fraction, but where  $f_p$  is taken to be the thermal equilibrium value for the local temperature. I call this case frozen equilibrium, although in the literature this is sometimes referred to as the intermediate specific heat.

To demonstrate the potentially profound effects that para hydrogen equilibration can have on thermodynamics, consider a parcel of hydrogen initially at deep levels where the temperature exceeds 300 K so that one has a 3:1 normal ratio, and move that parcel rapidly without equilibration up into the upper troposphere of a planet (for example, Jupiter where the temperature may be 120 K or so). Then let the parcel equilibrate through some process. Since this

is an exothermic process, the released heat produces a temperature increment. For example, on Jupiter it would be 4-5 K, on Saturn 8-9 K, and it becomes more and more pronounced the colder the upper troposphere is, so when we get to Uranus and Neptune it is quite a dramatic effect.

Let me just briefly mention some of the earlier historical work. In the mid-1960's, Larry Trafton (1967) first started worrying about the problem of the ortho-para ratio. He pointed out the importance of understanding this problem particularly in terms of the adiabatic lapse rate in the convective part of the atmosphere. Later, William Hayden Smith (1978) published a paper in which he emphasized the possible importance of the ortho-para ratio in the atmospheres of the outer planets and attempted to make some estimates of ortho-para ratios based on measurements of equivalent widths of the near infrared hydrogen quadrupole lines. To my knowledge, the first really quantitative attempt at modeling the behavior of para hydrogen fractions in the atmospheres of the outer planets is that due to Massie and Hunten (1982) in which they essentially made use of a one-dimensional aeronomical-type model with eddy mixing. They made various assumptions about para hydrogen equilibration times based on conversion mechanisms involving catalysis on the surfaces of cloud particles, and with this model they were able to calculate para hydrogen fractions as a function of altitude. I think it was really this work which inspired the more recent flurry of activity on hydrogen ortho-para ratio in the atmospheres of the outer planets. In particular, Gierasch (1983) published a brief paper in which he examined the potential dynamic implications of this. Basically in his model, he considered two adjacent atmospheric columns and allowed parcels to come up from depth at high temperatures (above 300 K) with a 3:1 normal ortho-para ratio. The parcels are then allowed to rise and then equilibrated up in the troposphere, releasing heat and producing a temperature increment, and then brought back down with a frozen para hydrogen fraction equal to that appropriate to the temperature near the tropopause. The parcel is then equilibrated again at depth. This produced a buoyancy contrast which in turn could support a thermal wind with velocities comparable to those observed on Jupiter. In fact, he did a similar model for Saturn where with its lower temperature, one can get an even stronger effect, and hence stronger zonal winds.

Largely through the urging of Gierasch and also Don Hunten, we attempted to examine the IRIS data from the point of view of what information we could get on the ortho-para ratio. Figure 2 shows the portion of the spectrum of interest here, between 200-600 wavenumbers, which covers the region of the two broad, collision-induced  $H_2$  lines, S(0) and S(1). The S(0) line corresponds to transitions between para states, and the S(1) line between ortho states. So to first approximation, the ratio of the strengths of the two lines is dependent on the ortho-para ratio. Since this is the thermal emission spectrum, one also must take into account the effect of the vertical thermal structure on the shape of the spectrum. Figure 3 represents calculations for three different para hydrogen fractions, with  $f_p = 0.25$  being that corresponding to normal hydrogen (the 3:1 case). The other extreme, 0.35, is approximately the local thermal equilibrium value for the upper troposphere of Jupiter. Clearly, there is substantial sensitivity, one line moving one way and the other line moving the other way. The effect on the S(1) line is less pronounced than that on the S(0) line simply because the S(1) line is formed in a region of

# VOYAGER IRIS

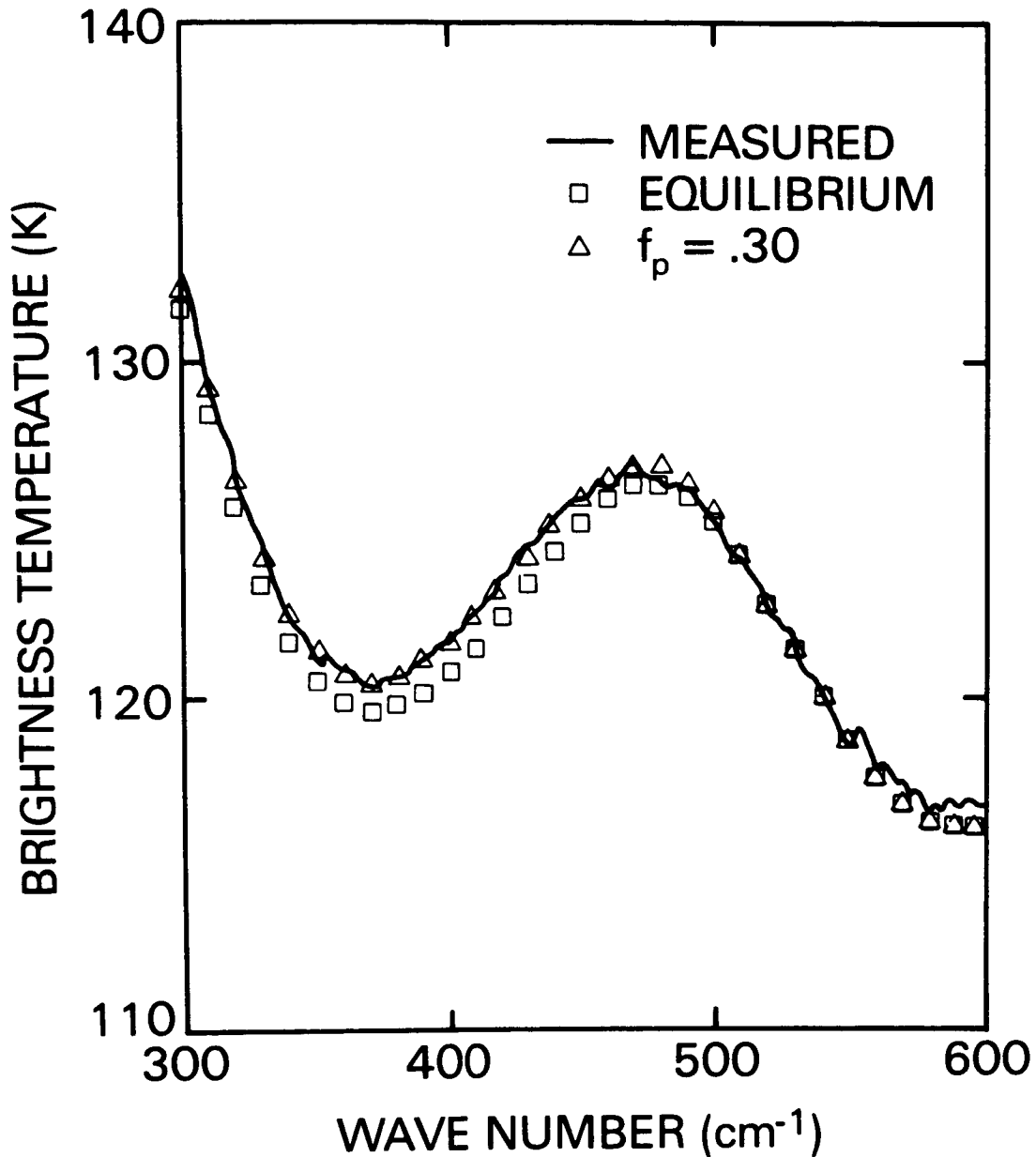


Figure 2. Comparison of the measured Jovian thermal emission spectrum with synthetic spectra calculated for equilibrium  $f_p$  and for a height independent value of 0.30. The theoretical spectra were calculated using the vertical temperature profile determined from the Voyager 1 ingress radio occultation. For this example, a height independent  $f_p = 0.30$  yields a better fit throughout the region of the S(0) line centered near 370  $\text{cm}^{-1}$ .

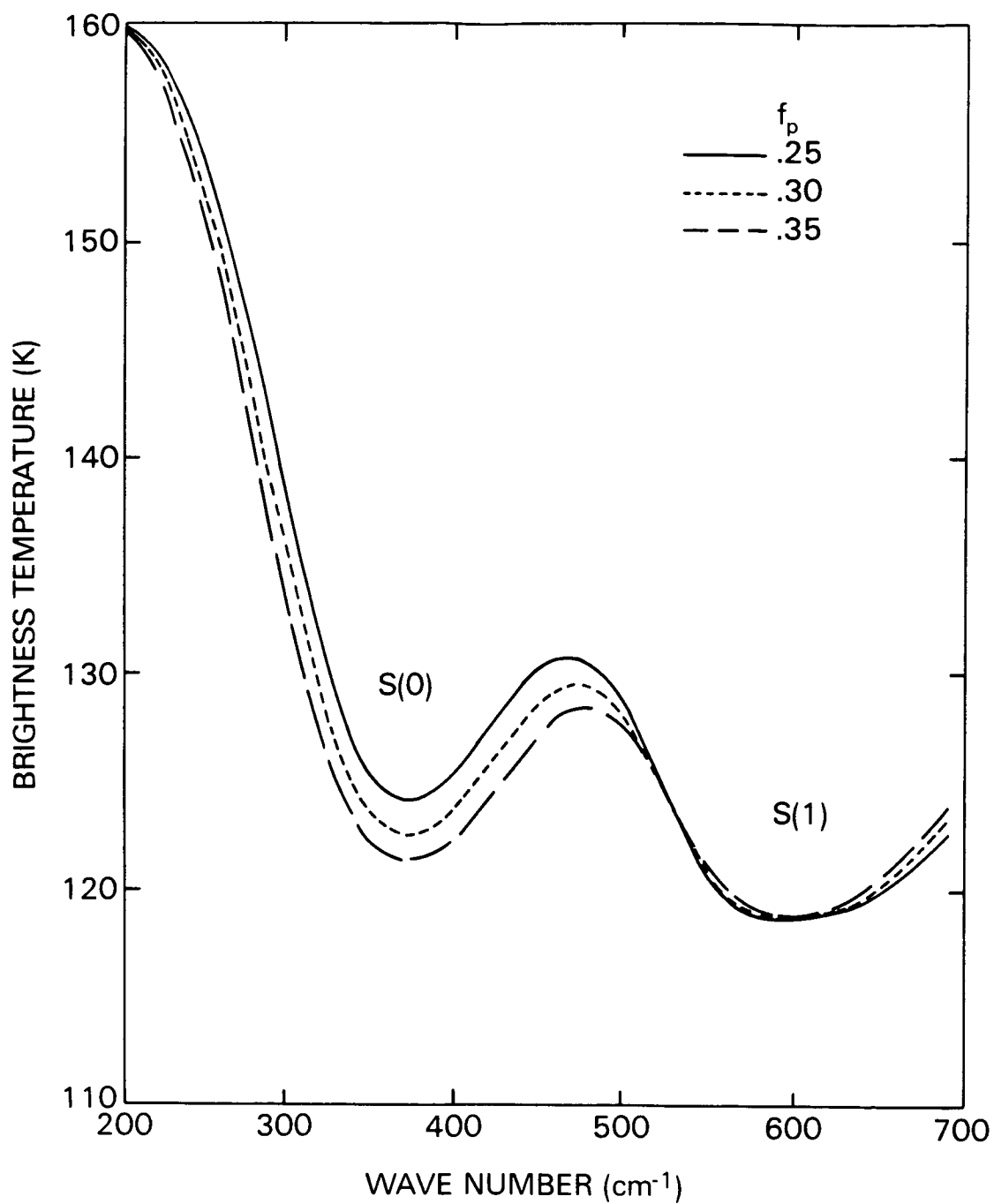


Figure 3. Theoretical Jovian spectra for three different values of the para hydrogen fraction  $f_p$ . By virtue of the opposing sensitivities of the S(0) and S(1) lines, the region near 520  $\text{cm}^{-1}$  is essentially independent of  $f_p$ .

relatively low lapse rates, so the measured gradient of brightness temperature is less sensitive to the opacity of the atmosphere in that region.

In particular, there is a crossover point between the two lines near  $520\text{ cm}^{-1}$  which is essentially insensitive to the ortho-para ratio, but rather is sensitive only to the thermal profile in the atmosphere, specifically the temperature in a layer centered near 250 mb. The temperature can be gotten for that region from this point and then by choosing another point on the spectrum, such as that near  $320\text{ cm}^{-1}$ , which has a similar nominal optical depth and which is sensitive to the para hydrogen fraction, one can then make a quantitative estimate of the para hydrogen fraction.

We essentially did this with a large volume of Voyager data, and Fig. 4 illustrates the basic result. This set of data was taken from a mapping sequence covering the entire planet from about  $\pm 60$  deg latitude. Figure 4 shows the retrieved values of  $f_p$  from 536 individual spectra subjected to a running mean with latitude bins five degrees wide and indicates the systematic variation of the para hydrogen fraction as a function of latitude. The normal case would be down at the bottom of the graph corresponding to a para hydrogen fraction of 0.25. If in fact, the atmosphere had been in local thermal equilibrium at the local temperature, we would have anticipated retrieving values corresponding to the solid curve labeled "equilibrium." The reason that this is a function of latitude is that you are looking at different emission angles at different latitudes, so therefore we sample slightly different levels in the atmosphere, and the equilibrium para hydrogen fraction being a function of temperature, will be different for the different levels.

The principal feature of the para hydrogen fraction is its large scale latitudinal variation. Although there are hints of other structure, there does not seem to be any distinct correlation with any other planetary-scale parameters. For example, it does not seem to correlate in any consistent way with the jet structure on the planet. It does not correlate with any of the indicators of the presence of clouds such as temperature in the  $5\text{ }\mu\text{m}$  region or measurements of brightness temperature for the continuum between the ammonia lines in the  $50\text{ }\mu\text{m}$  region. There are of course always concerns with this type of inference. Are there other opacity effects which are affecting it? If there are cloud opacity effects acting, it must be in a very complicated way because we see no direct correlation for clouds. While the result is sensitive to differential errors in the absorption coefficients, which could in fact lead to systematic errors, it seems doubtful that they would produce a latitudinal-type gradient such as we see here. We seem to be stuck with this large-scale gradient requiring some explanation.

If we attempt the same thing for Saturn, we find that it is more difficult to do the analysis because of lower signal-to-noise at colder temperatures. Also, the upper tropospheric lapse rate on Saturn is lower, so there is less contrast in the lines. Still, to the extent that we've been able to analyze Saturn data, we find, unlike the case for Jupiter, something that appears to be close to equilibrium.

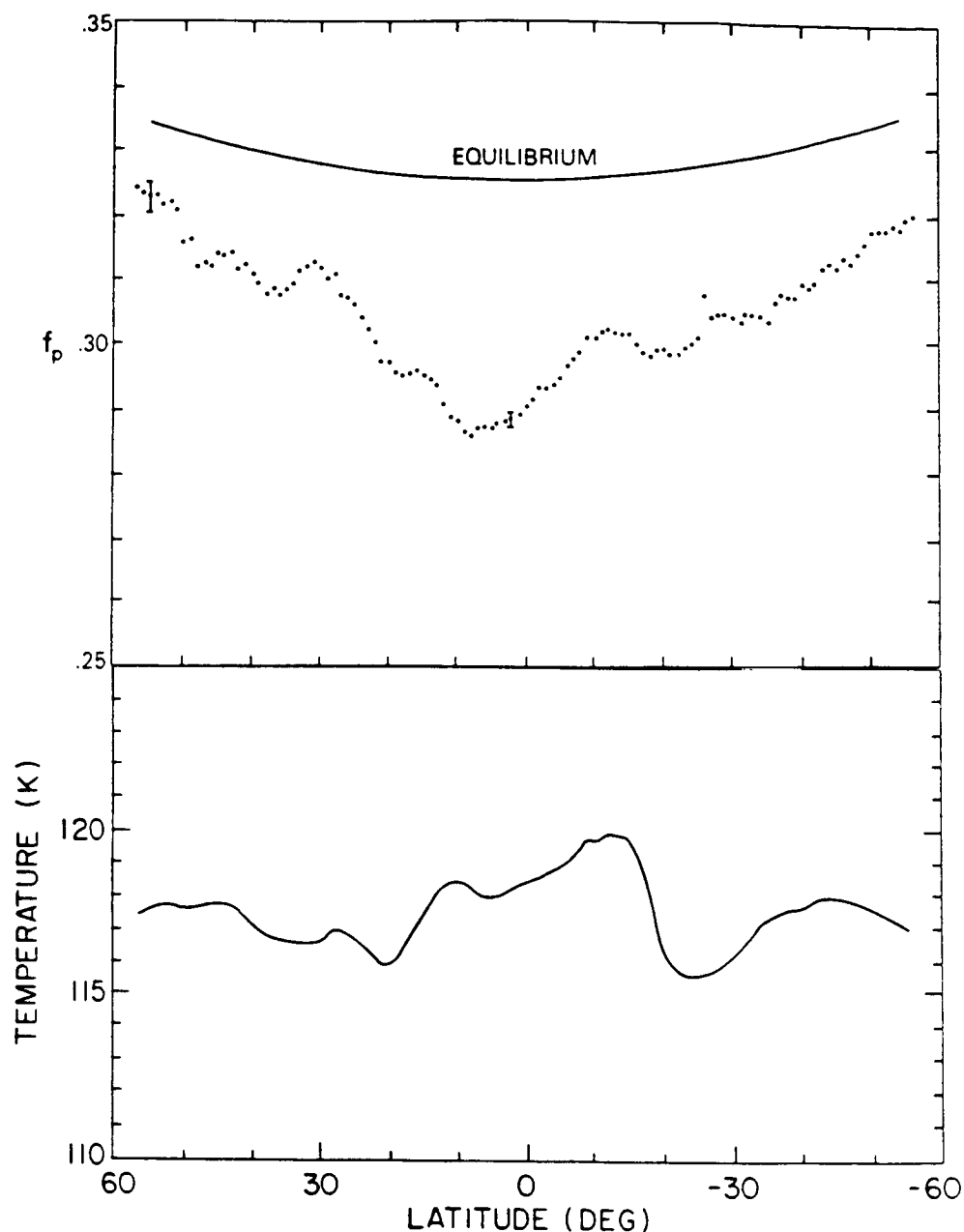


Figure 4. Comparison of inferred para hydrogen fraction and temperature as a function of latitude. The upper panel displays points which correspond to zonal averages of individual  $f_p$  determinations over latitude intervals of five degrees calculated at increments of one degree. If the equilibrium value of  $f_p$  prevailed at each atmospheric level, the results would lie along the indicated solid line curve. The lower panel shows the meridional temperature profile corresponding to the 270 mb level in the atmosphere.



What can we say about the nature of this sort of distribution for the ortho-para ratio? Basically, one looks for a dynamical explanation. We have to explain how the dynamics can have a profound effect on the para hydrogen fraction. At the same time those dynamic processes do not significantly affect the temperature field at this same level. Figure 4 also shows the mean zonal temperatures derived from Voyager data for the same global mapping sequence. Temperature for a layer at 270 mb is displayed as a function of latitude. Here of course, temperature variations are relatively modest, in the vicinity of 3 to 4 degrees at most. How do we perturb the para hydrogen fraction but leave the temperature field relatively unperturbed? One possibility is the following: suppose we have a large-scale meridional circulation with upwelling at the equator and downwelling at the higher latitudes. The rapidly upwelling gas would bring hydrogen up with a lower para hydrogen fraction associated with the warmer temperatures at depth. If the dynamic time scale corresponding to the upwelling is comparable to or less than the para hydrogen relaxation time scale, then the observed perturbation from the local equilibrium value could be produced for the equatorial region. Subsequent transport towards the polar regions with continuing equilibration of the para hydrogen fraction would then be consistent with the  $f_p$  values closer to local equilibrium at high latitudes.

Such a scenario would work qualitatively for the para hydrogen fraction, but in order for such a circulation system to leave the temperature field relatively unperturbed, the dynamic time scale would have to be longer than the radiative relaxation time. So this puts constraints on the ratio of the equilibration time of para hydrogen to the radiative relaxation time. Specifically, the para hydrogen relaxation time would have to be longer than the radiative relaxation time. If we take as an upper limit on the hydrogen relaxation time, that for pure hydrogen, an appropriate estimate for the 500 mb level on Jupiter is about  $10^9$  seconds. The radiative time constant there is about  $10^8$  seconds, so in that case the para hydrogen relaxation time would exceed the radiative relaxation time. On Saturn, however, they would be about the same order, so we would expect much less perturbation from equilibrium in the para hydrogen fraction on Saturn. As we go out to Uranus and Neptune, it should be even closer to equilibrium because of the relation of the two time scales.

Obviously, we don't know with certainty what the hydrogen relaxation time is. It's not well-constrained; this is one of the big unknowns of the problem. We don't know what the relaxation time of molecular hydrogen is in the presence of ammonia ice crystals, for example (which is something I hope someone will eventually estimate). The other feature of this type of model is the embarrassing fact that the jet scale does not seem to enter into it, and it is hard to conceive of a jet system as not having some meridional circulation associated with it. So why do we not see the influence of the jets in the para hydrogen fraction? An interesting question is whether the para hydrogen fraction is really acting as a quasi-conserved Lagrangian mean tracer for the motion or not.

That is the situation for the observable part of the atmosphere. We can also make some speculative comments on the deeper layers, the convective part of the atmosphere. The ortho-para observations pertain to the upper, stably

stratified portion of the atmosphere. In the deeper levels, we presumably have buoyancy-driven convection. What is the effect of the para hydrogen equilibration in this region? We have attempted to look at that question within the framework of fairly simple mixing length theory. Basically, what one has to do is devise a mixing length theory in which one treats the coupled problem of the transport of the para hydrogen fraction and the heat transport with the constraint being that the heat flux should agree with what is observed. If you do this sort of thing, and assume that the para hydrogen relaxation time is long in the sense that it is close to that of pure hydrogen (which in the deeper layers would probably be  $10^7$  to  $10^8$  seconds) then one finds that the efficiency of the convection is enhanced considerably due to the lag in the conversion that occurs in the process. If you do just a standard mixing length calculation, then you come out with an overturning time on the order of  $10^4$  seconds, while if you include a lag in the para hydrogen conversion, the overturning time lengthens by three orders of magnitude, with a substantial increase in the efficiency of the convection. The para hydrogen conversion acts in an analogous way to latent heat release by water in the Earth's atmosphere. In this case, a substantial fraction of the heat flux would be carried in the form of this latent heat, the energy difference between the ortho and para states. Due to the slowness of the convection, one would come out with a para hydrogen fraction very close to the local thermal equilibrium value and with a lapse rate close to an equilibrium lapse rate. If we consider high-frequency perturbations on such a state, that is displacements of fluid parcels on a time scale short compared to the basic convective scale, the atmosphere would be statically stable to such perturbations because the parcels would essentially follow adiabats which correspond to the frozen equilibrium specific heat, which is a steeper temperature gradient than the equilibrium adiabat. Therefore, such perturbations would in fact be stable in the sense of having a positive value for the square of the Brunt frequency.

This again is mostly speculation, because we don't know what the equilibration times are at these levels. It is based on the assumption of a relatively slow equilibration time, something like that approached by pure hydrogen. This kind of prediction would be made not only for Jupiter but for all of the outer planets including Uranus and Neptune. The lapse rate can't really be checked on Jupiter and Saturn because at those temperatures the equilibrium adiabat, the frozen equilibrium adiabat, and the normal adiabat all lie rather close together. So it is not possible to distinguish these gradients from the available data. As you go to the outer planets, this is no longer the case.

What do we know about Uranus and Neptune? There are several pieces of information available. There have been analyses on, for example, the 4-0 S(0) and S(1) quadrupole lines of hydrogen for these planets. In fact, there is a paper later in the meeting by Baines and Bergstralh on this problem. It will be interesting to hear their latest results. If I understand it correctly, they conclude that the data are generally consistent with an ortho-para ratio closer to the equilibrium than normal. This appears also to be true for a longer wavelength measurement by Orton, Tokunaga, Moseley and others with a spectrum that is at least inconsistent with normal para hydrogen fraction, and probably more nearly consistent with something closer to equilibrium. On the other hand, measurements in the sub-millimeter and millimeter part of the

spectrum, which on Uranus and Neptune penetrate down into the convective regions of the planet, seem to be consistent with a temperature lapse rate which corresponds not to the equilibrium, but to the frozen adiabatic lapse rate. This, at least in my opinion, is hard to understand. It is hard to understand how you would have processes which would be establishing a frozen equilibrium lapse rate, which would require motions which are rapid compared to the hydrogen equilibration time, but at the same time able to maintain an equilibrium para hydrogen fraction at that level. It would seem to me that the processes of establishing both a para hydrogen fraction and a temperature lapse rate are most likely one and the same. It is hard to understand how the two different time scales could be operative here. There may be situations in which one could have equilibration occurring in multiple cloud layers (Massie and Hunten alluded to this sort of thing in their paper) where in between one might have a para hydrogen fraction which closely resembles an equilibrium fraction, but nevertheless equilibration would not be occurring on the time scale of the mixing.

In any case, this at least is an open question and one of the big unanswered questions in this area. These observations have all been full disk observations. It will be interesting to try to get spatially resolved observations and Voyager will hopefully do this in January 1986. The Voyager infrared spectrometer should be able to obtain spectra in the region from 200 to 400  $\text{cm}^{-1}$  with sufficient signal-to-noise to permit quantitative work. Theoretically calculated spectra indicate that the difference in the shape of the spectrum in this region between the two extremes, normal and equilibrium, is large enough so that it should be possible to make statements at least on the gross para hydrogen fraction, hopefully, as a function if not of detailed spatial resolution on the planet, at least on a regional basis.

Let me then just summarize with a list of what I regard as the current questions in the area. I am sure there are other things to be added to this list. One of the big questions is what really is the para hydrogen equilibration time in the presence of ammonia and methane ice clouds? Until one gets a better answer to this question, it is hard to do much more than speculate on this problem at least in terms of models. What is the mechanism for producing large-scale variation in  $f_p$  on Jupiter, and is it really a variation in  $f_p$  that we are seeing? I think it's important to get a determination of para hydrogen fraction by other techniques such as ground based measurements in the near infrared hydrogen lines. Work is being done by one or more groups on this area. How important is the latent heat flux in the convective region? We are talking about the region essentially above the 250 K level; this is the only part of the convective region where the para hydrogen fraction is really important. One gets a significant latent heat effect between that level and the top of the convective region. What is the mechanism producing the apparent equilibrium para hydrogen fraction on Uranus and Neptune but a frozen equilibrium lapse rate? That seems to be in conflict with the time scales which one would need. Are there other dynamical processes for which the para hydrogen equilibration is important? This, after all, is perhaps the Jupiter equivalent to moist latent heat processes in the Earth's atmosphere. There has been very little thought given as to exactly how this para hydrogen latent heat process can enter into other dynamical processes on the planet.

## REFERENCES

- Gierasch, P. J. (1983). Dynamical consequences of orthohydrogen-parahydrogen disequilibrium on Jupiter and Saturn. *Science* 219, 847-849.
- Massie, S. T., and D. M. Hunten (1982). Conversion of para and ortho hydrogen in the Jovian planets. *Icarus* 49, 213-226.
- Smith, W. H. (1978). On the ortho-para equilibrium of  $H_2$  in the atmospheres of the Jovian planets. *Icarus* 33, 210-216.
- Trafton, L. M. (1967). Model atmospheres of the major planets. *Astrophys. J.* 147, 765-781.

DR. POLLACK: If I understand correctly, Peter's original motivation here was to reproduce the pattern of zonal bands. He used the observed variation in the ortho-para ratio to infer temperature variations  $\Delta T$ , which in turn produced strong thermal winds comparable to observed values. So, if you, in your observations, don't see a significant  $\Delta T$ , there is a kind of breakdown in the logic for the whole picture.

DR. CONRATH: That theory, I think, is of historical interest at this point. I used it mainly to illustrate the potential. The observations are not consistent with that picture.

DR. HUNTEN: Well, I'd say we've got to be a little wary of believing all the observations in this area because it is very, very difficult. The observations are fine, but the interpretations may be questionable.

DR. INGERSOLL: Do you believe the observations and implications about this large-scale meridional structure? Couldn't you have enhanced vertical mixing at the equator on a smaller scale without the large-scale meridional structure?

DR. CONRATH: This is one of the things we looked at. You also have to keep the heat flux as a function of latitude correct. It seemed to us that since you have a maximum solar heating at low latitudes and less solar heating at high latitudes, that you would get in trouble with heat flux with this type of picture.

DR. HUNTEN: When you say heat flux, you mean thermal IR radiation flux.

DR. CONRATH: Yes, the heat flux, the radiation to space.

DR. W. H. SMITH: First, as far as ortho-para conversions are concerned, it is well understood that ammonia and methane crystals in an undamaged state do not catalyze the relaxation of the hydrogen into an ortho-para equilibrium. Radiation damaged crystals or crystals with paramagnetic impurities may act as a catalyst to induce the equilibrium. As observations have progressed, since 1978 when I wrote my paper on this subject, we have been convinced that some disequilibrium may in fact be present, but primarily in the regions above, say

... 4

100 mb. This disequilibration may be photochemically induced, and I wonder why that has not been discussed, particularly with regard to the high regions. It would seem to me that the vertical convection processes will control the equilibration process in the bulk of the atmosphere. Vertical transport of  $H_2$  does not seem to be fast enough to displace the equilibrium very far over most of the observed pressure range.

DR. CONRATH: We were thinking of the dynamic effects from the point of view that perhaps you do have equilibrations occurring efficiently up to some point, perhaps to the top of the main ammonia cloud deck. But the region we're seeing is actually somewhat above this, just below the tropopause, where if you have sufficiently rapid upwelling, you will see the para hydrogen fraction associated with that region where it was last well equilibrated. This was the reason for looking at this type of model.

DR. ATREYA: Barney, if I remember correctly the  $\Delta T$  is equal for both Uranus and Neptune, around 9 K. Would the absence of an internal heat source on Uranus make any difference?

DR. CONRATH: Not in terms of that simple thermodynamic calculation, but it obviously will in terms of the dynamics if there is not a strong heat flux. Gierasch and I made predictions on Uranus which were an extrapolation from Jupiter and Saturn, and these are probably not relevant in retrospect. I think that all bets are off here because you have such a small internal heat source and such a large obliquity and strong seasonal driving.

DR. ORTON: I'd like to respond to that in part because we are seeing the same frozen equilibrium signature in the sub-millimeter region for both Uranus and Neptune. So this question seems to be rather divorced from the internal heat differences which are substantial between the two planets.

DR. ROSSOW: On your synthetic spectrum, would you comment on how well the synthetic spectrum actually fits your results?

DR. CONRATH: I do not have the figures showing the comparisons with synthetic spectra with me, though they are given in two different publications that describe this. Basically, you get a poor fit at low latitudes if you assume equilibrium and substantially better with a disequilibrium ortho-para ratio.

DR. ROSSOW: What do the remaining differences reflect?

DR. CONRATH: There are still differences in the spectra greater than the statistical noise, particularly in the center of the  $S(0)$  line.

DR. ALLEN: When you showed the IRIS results of latitudinal variation, you made the observation that it didn't seem to match the observed pattern of belts and zones. The one thing it does seem to match is the UV image of Jupiter at 2400 Å from Voyager, where there is this correlation between high haze and near equilibrium. I am wondering whether this says anything about the conversion mechanism. Instead of talking about the main ammonia cloud deck, maybe we should talk about the properties of equilibration in the haze layer. Something that produces UV absorption might have characteristics

appropriate for doing the equilibration. Then you see a very nice tie-in between a mechanism for equilibration and possibly something observed there.

DR. CONRATH: Yes, if you look at it from this point of view, this would require less equilibration at low latitudes and more at high latitudes.

DR. ALLEN: Which is what you see.

DR. CONRATH: Your haze levels are pretty high, are they not?

DR. ALLEN: Between 50 and 150 mb.

DR. WEST: I would like to elaborate on that comment just a little bit. One possible way of explaining the distribution of stratospheric aerosols is by looking at the dynamics and diffusion of those aerosols. It's really hard to differentiate anything.

DR. HUNTEN: Barney did comment that it would be nice to have lab measurements of the effects of UV irradiated aerosols, and I would certainly agree. It would cut down at least one degree of freedom in this problem.

